

X ↓ P/4  
O & JSM/164.

MAY 14 1992

Dr. Joshua Lederberg,  
The Rockefeller University,  
1230 York Avenue,  
New York, NY. 10021-6399

May 8, 1992.

Dear Dr. Lederberg,

This is in reply to your letter postmarked March 30 inviting reminiscences of the 1946 Cold Spring Harbor Symposium when you presented your paper on gene recombination in E. coli. I am now retired and visit the McMaster Biology department only once every month or so to see if there is any mail. That is why my reply is so late.

First, I thank you for the reprint on yeast heterokaryons enclosed with your letter and also for the copy of your 1946 paper. I can not send you a copy of the apomixis review that you requested, since Academic Press supplied me none. But I am enclosing a copy of a review I wrote on yeast sporulation after retirement which may be of use to you. My goal was to cover the main aspects of the subject in an up-to-date manner and incidentally to tell biotechnologists what they need to know about yeast sporulation.

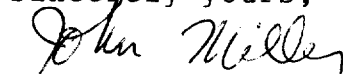
- Yeasts v. 3 (1989)

We discovered 3 apomictic yeast strains in my laboratory in 1972 and supplied them to some other laboratories. A modest literature built up about them which appeared to merit a review and Academic Press asked to have the review include other eukaryotic microorganisms as well. As a result we ended up with the first review to survey apomixis in eukaryotic microorganisms and relate them to higher forms in this regard. I think yeast apomixis provides an excellent system for critical research on apomictic mechanisms and also for population genetics and yeast ecology as section VIII of the review points out.

Concerning the 1946 CSH Symposium, two things stand out in my memory. [1] It was the only scientific gathering I ever attended where I slept in a tent. I shared one with Dr. A.R. Gopal Ayengar. [2] Your paper on E. coli recombination. At the time I was a mycologist and did not fully appreciate the significance of your findings but realized from the keen interest of the audience that it was very important. I recall your debate with Dr. Lwoff and my impression there was that Dr. Lwoff was not actually challenging your interpretation of data but was constructively suggesting things you could do that would remove any shadow of a doubt that recombination was occurring. However, you seemed to feel his attitude was negative and your spirited defense led Dr. Tatum to intervene diplomatically to harmonize the situation. I also recall Dr. Delbruck questioning you but no details persist.

Unfortunately, I have no notes or other records of the meeting.

Sincerely yours,



J.J. Miller,  
54 Whitton Rd.,  
Hamilton, Ont., Canada, L8S 4C7.